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ON
THE THERAPEUTIC VALUE
OF
NERVE-STRETCHING
IN
TABES DORSALIS.

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DUBLIN:
PRINTED FOR THE AUTHOR
BY JOHN FALCONER, 53, UPPER SACKVILLE-STREET.

1883.

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ANY mode of treatment for tabes dorsalis, or, as it is more frequently termed in this country, progressive locomotor ataxy, which may even temporarily relieve some of its symptoms, though as yet the *rationale* of such treatment remains unexplained, is deserving of consideration and study. That nerve-stretching has been in many cases attended with benefit, which in some few has been permanent, cannot be denied. The evidence afforded by the cases of Carl Langenbuch, Prof. Esmarch, Erb, Müller, Eulenburg, Grainger Stewart, Charlton Bastian, Cavafy, G. Buchanan, Gillette, and others, as well as the statistical records compiled by Dr. Chandler of New York, all point to the fact that relief from some of the most distressing symptoms in ataxy may, if the operation be skilfully performed, and at a comparatively early period of the development of the disease, be reasonably anticipated. In most cases, not merely relief from the characteristic lightning pains is obtained, but in a few ataxic improvement has been observed. So far from any satisfactory physiological explanation having been given as to the way in which the operation acts, there has not even as yet been any very busy speculation on the subject; and perhaps it is just as well this should be so, speculation being of little use so long as the chances of verifying or disproving theory are remote. We need not, however, reject a line of treatment because its *modus operandi* is as yet incapable of explanation. Already we acknowledge the great value of many methods of treatment employed by the physician which are essentially empirical, and which modify beneficially the processes of disease. In surgery, too, we must admit that certain operative measures are attended with results of which, in our present state of knowledge, we are unable to give satisfactory explanation—for example, the manipulations necessary for the relief of the alleged displacement of the internal semilunar cartilage of the knee-joint; acupuncture in relieving certain painful affections, notably of the os calcis, and iridectomy in glaucoma. To these must now be added nerve-stretching in tabes dorsalis.

Two well-marked examples of this disease were recently under my care in the Richmond Hospital. In one of them I stretched the sciatic nerve on one side, in the other on both, and the results, though the operations were not followed by any signal improvement in motor power, were certainly in other respects satisfactory and encouraging.

Case I.—The first of these was that of a male patient aged thirty-two, who was under my care during March, 1881. He was a tall, well-built, broad-shouldered, muscular individual, whose life had been regular, steady, and temperate. The disease manifested itself first about two years previously to the above-mentioned date, and the patient attributed it to exposure to cold. There was no history of his having contracted syphilis, nor was there evidence of his having inherited it. He had, previously to his admission into the Richmond Hospital, been in three other hospitals in this city, under the care of physicians of recognised ability. Various lines of treatment had been adopted, all with unvarying want of success, the symptoms steadily getting worse as time went on. These were well marked when he came under my care—ataxic gait, plantar anæsthesia, inability to walk with his eyes closed or in the dark without assistance, patellar reflex absolutely wanting, and the sudden drop into a chair when sitting down. There was much vesical irritability and frequent lightning pains of exceptional severity. The case did not illustrate particularly well the Argyle Robertson test, as the pupils did react to light, though sluggishly. Sexual desire was quite extinct. The chief sources of distress to the patient were the lightning pains and vesical irritability. Having regard to the fact that so many of the methods of treatment usually adopted in this disease had been already tried, it appeared to me to be one for which nerve-stretching might possibly be of service. Accordingly I suggested the operation to the patient, taking care at the same time to let him understand that I was not particularly sanguine as to any very marked good result being obtained from it. Having experienced, however, the inutility of all previous lines of treatment, he was only too glad to learn of one that afforded him even a remote chance of getting relief, and pressed me to perform the operation. Accordingly, on March 7th, I exposed the right sciatic nerve, and, passing my left index finger underneath and round it, I drew it forwards with an amount of force which I cannot estimate accurately, but which I should say was not more than 8 or 10 lbs. at most. Not only did I draw the nerve forwards but also downwards, so as to make a direct traction on the cord. The operation was performed with strict antiseptic precautions. I shall not weary my hearers with an account of the daily progress of this case. The healing of the wound, though it remained aseptic throughout, was, as is usual in these cases—a circumstance noted by Dr. Cavafy—exceptionally slow, but was ultimately complete; and as regards the outcome of the operation, I may mention that although there was no improvement in locomotive power, no return of patellar reflex, and no change in his power of walking in the dark or with eyes closed, yet still, in three of the characteristic symptoms of the disease, there was a very marked change observed. The first of these was a restoration of plantar sensibility; the second, a marked diminution both in frequency and intensity of the lightning pains; and lastly, the vesical irritability ceased almost entirely. This cessation, however, I regret to say, was only temporary, for six weeks after the operation there was a distinct return of it. Before the patient left hospital he expressed a great desire, after a short time, to have the operation done on the left side; but I heard afterwards he was dissuaded from it by his friends.

What may be said, therefore, of the result of the operation in this case is, that it exercised a distinct influence on the disease, one of the most distressing symptoms the patient had—that referable, namely, to the bladder—being, for a considerable period, distinctly improved. As regards the relief obtained from the lightning pains and localised anæsthesia, I am not in a position to say whether it was permanent or not, but at the time the patient left the hospital there was no evidence of their return. The result obtained in the following case, the notes of which were taken for me by my former clinical assistant, Mr. Cassidi, was much more remarkable:—

Case II.—The patient, formerly a soldier, a man of medium height, slender figure, and fairly good muscular development, was admitted into the Richmond Hospital, under my care, on January 30, 1882. In June, 1881, while on parade, he fell suddenly on the ground. He had felt his legs "failing" for three months previously, and during the previous winter had suffered from shooting pains and cramps in the calves of his legs and thighs, which attacked him nearly every night, and in the morning while putting on his boots. In the following August these pains became more severe, but still were confined to his legs. About October he experienced difficulty in passing water, often trying in vain for half an hour to do so, and then when he desisted from the effort frequently found the urine dribbling down his legs.

His walking was very peculiar, hurried, his heels first and with some force striking the ground. With his eyes closed he found it wholly impossible to make a step forward. There was no muscular wasting. He complained that the sense of touch in his hands was less acute than formerly. His sight was unaffected. During his 19½ years military service he was 3½ years in Canada, and subsequently 3 years in the Bermudas. The climate here he found exceptionally good. During his lengthened period of service he only suffered hardship once, and that was during the autumn manœuvres at Aldershot in 1876, when for ten days he was out under heavy rain each day, and was wet through. After this he stated he felt stiff in all his joints. He then entered the military hospital, where he remained for a month.

I may mention that in this case there were no regions of anæsthesia, no sensations of constriction; but complete absence of patellar reflex, and also the pupillary contraction during accommodation, though not to light (the Argyle Robertson test), was well marked. There were occasional attacks of retention of urine, and the bowels were habitually costive. On February 8th I exposed and stretched both sciatic nerves, drawing each nerve forwards and downwards, and then releasing them suddenly. Severe and constant pain at the situation of the operations, accompanied by spasmodic contraction of the muscles of the leg, were complained of by the patient for some hours after the operation. These were relieved by hypodermic injections of morphia. During the following three days these recurred with varying intensity, and were treated by morphia injections and draughts containing chloral hydrate, bromide of potass. and opium. On February 20th a marked change for the better is noted. The patient has completely lost the "lightning pains," there is increase of power in his legs, he has no urinary trouble, and the bowels are regular. On April 1st it is reported that there had been no return of the lightning pains, but no marked improvement in his walking or in the numbness of the fingers. On the 14th April it is noted that the power of walking is much improved; no return of the pains whatever, though there is of the constipation. On the 19th the patient expressed much satisfaction at the complete relief from the urinary and vesical troubles and "lightning pains" that he has enjoyed uninterruptedly from a few days after the operation up to the present time.

It will thus be seen that in most respects the results obtained in these cases mainly coincide with those already noted by the operators, who have had experience in this method of treatment. This is particularly the case as regards the relief from lightning pains. In a paper by Dr. Chandler,[a] on Nerve-Stretching, will be found a statistical record of 67 cases of locomotor ataxy in which this treatment was adopted. Of these 55 are reported as improved. 25·1 temporarily improved, and 14·9 as failures. All the operators bear testimony as to the relief obtained from the lightning pains.

If the peripheral origin theory of this disease—one which has so zealous an advocate in Carl Langenbuch, the originator of the operation for tabes dorsalis—be accepted, it would doubtless be easier to understand the *rationale* of the treatment. The evidence in favour of this, however, is far from satisfactory, and is in fact opposed to most clinical as well as pathological experience—the only British authority of eminence that I am aware is disposed to accept the theory being Prof. Grainger Stewart, who to a certain extent endorses Prof. Langenbuch's theory, maintaining that in some cases of this disease changes possibly occur primarily in the nerves and secondarily in the cord, and that the benefit resulting from the operation may turn out to be due to removing changes in the nerves themselves. This view, however, is certainly at variance with what is held by the great majority of pathologists.

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[a] London Med. Record, Dec. 15, 1882.

As to what the effect of stretching on the nerves as distinguished from that on the medullary axis may be, there is among physiologists a remarkable difference of opinion. In a recently published memoir by Ceccherelli,[a] the view is maintained that nerve-stretching causes loss of sensibility from interruption of the ascending sensory current, the descending or motor current being preserved, and that in moderate nerve-stretching anæsthesia in the territory of the nerve is produced without loss of motility, and when the force used is excessive persistent anæsthesia and alterations of motility and nutrition are observed. These results do not coincide with what was noted in the cases I have detailed, nor with the experience of Dr. Brown-Séquard, who has shown that in cases where there was anæsthesia, after stretching the sciatic nerve, hyperæsthesia was induced, and a diminution of feeling when there was hyperæsthesia.

As regards other affections for which nerve-stretching has been employed, I cannot speak from my own experience; but I may mention that I have been impressed by a result of nerve-stretching obtained recently by my colleague, Dr. Corley, in a case of sciatica in which the patient's sufferings were prolonged and extreme, the etiology was obscure, and which had resisted, previously to operation, many of the more usually adopted lines of treatment. The success obtained in this case was remarkable. The patient's acute suffering ceased soon after the operation and has not recurred.

In connexion with nerve-stretching, one of the most important considerations for the surgeon is, I think, to estimate the amount of force that should be used in the operation. There can be no doubt that many of the recorded cases of failure have been due to this estimate not having been made, and in some cases the result has been that the amount of force employed was so great as to cause the nerve to be permanently damaged, and gave rise to troubles which eventually led to the patient's death. In all future cases in which I may be called on to perform the operation, I purpose employing a dynamometer with the view of determining the amount of force that may be used. This has been already done by M. Gillette, who has devised an instrument which he has termed a "nerve-stretcher." One feature in this appliance, however, would make me hesitate to use it, which is, that the nerve is stretched over a blunt metallic hook, the employment of which would, I think, be likely to be attended with local injury to the nerve. Dr. Cavafy also objects to M. Gillette's "nerve-stretcher," on the ground that the "extension must affect the peripheral and central portions of the nerve itself." I agree with Mr. Symington in thinking this no valid objection, there being less liability to rupture, "as the force is divided," the latter authority observes, "between the peripheral and central ends." The area of nerve stimulation too is greater when both peripheral and central portions of the nerve are stretched. The plan I would be disposed to adopt would be to pass a thick round caoutchouc cord underneath the nerve and attach its free ends to a dynamometer. The force applied in this manner would, I think, entail the minimum of risk to the nerve and would enable the operator to estimate with mathematical accuracy the amount of force employed; it need not, I believe, be very great to obtain the required therapeutic results. In the two cases I have mentioned, I cannot state accurately what amount of force I employed, but I feel confident from experiments I have since made it did not exceed 8 or 10 lbs. I was much surprised, therefore, to learn that M. Artant (*Rev. de Chir.*, Feb., 1882) recommends that the force to be employed to the sciatic nerve should not exceed 40 kilogrammes; and for other and smaller nerves 20 kilogrammes. The application of a force so great must be attended with great risk of permanent damage to the nerve, a damage resulting from hæmorrhage, rupture of nerve fibres, or even complete separation of the nerve from its spinal attachments.

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[a] London Med. Record, April, 1883.

It is not unlikely that many of the cases in which the reverse of improvement has been the outcome of the operation is to be attributed to the employment of undue violence. For example, Dr. Kulenkampff (*Berl. Clin. Wochen.*, 1881) records a case of ataxy in a barber aged forty-three, in whom both sciatic nerves were stretched, the left with such force that the body was raised. The day following the operation, cystitis supervened, which the author considered, and doubtless with reason, neuroparalytic. The patient died seventeen days after the operation. The condition, according to the operator, was similar to what occurs after fractures of the spine, the roots of the sacral nerves having been so much injured that the vesical nerves were functionally destroyed.[a] Dr. Althaus[b] has mentioned five other fatal cases, those, namely, of Socin, Langenbuch, Bilroth, Berger, and Benedict. In most of those cases it is stated "the cause of death appears to have been undue violence used in stretching, whereby the medulla oblongata would appear to have received a shock. Thus in Benedict's case, severe vomiting and singultus, together with complete paralysis of the bladder and bowels, supervened after the operation; dyspnœa and cyanosis eventually set in, and the patient died comatose on the ninth day." These facts are sufficient to show how cautious the operator should be not to use any excessive force.

In Mr. Symington's paper on "The Physics of Nerve-Stretching," he gives the results of nine experiments he made, with the view of obtaining a reliable guide as to what amount of force the surgeon might with safety apply. In two of these experiments a force equivalent to 100 lbs. was sufficient to sever the connexions of the nerve with the spinal cord. In another case the same result was obtained after the application of a force of 84 lbs. In the other cases the force required for either rupturing the nerve at the point where the force was applied, or tearing its connexions with the spinal cord, was in excess of the forces already mentioned. I cannot but think that for therapeutic purposes a force far below one likely to be attended with so calamitous a result as rupture or tearing of the nerve from its spinal connections would be amply sufficient, and that, therefore, one not even appoaching what has been suggested by some operators, notably M. Artant, should be employed. The operation has been followed by other accidents besides nerve-rupture, hæmorrhage, and those already mentioned—these are, venous thrombosis (Socin) and erysipelas (Erlenmeyer). These accidents, however, nerve-stretching has in common with so many other surgical operative measures, that they hardly deserve even a passing notice.

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[a] London Med. Record, March, 1882.

[b] British Med. Journal, Jan. 7, 1882.

Another important point raised by Mr. Symington is in reference to whether stretching—employing, I presume, a moderate amount of force—produces any mechanical effect on the spinal cord. He believes from anatomical considerations that it does not, "the nerve fibres as far up as the dura mater being put upon the stretch, but not the spinal nerve roots beyond."

Although as yet we "see through a glass darkly" as to how the beneficial effects of nerve-stretching are produced, a satisfactory physiological explanation being still looked for, as to whether, namely, in central lesions it plays the *role* of a counter irritant, acting indirectly on the cellular elements and nutrition of the cord, or whether the changes consequent to the operation are due to local nerve irritation alone, are questions which still await solution. Still we are not wholly in the dark. We know, as Dr. Brown-Séquard, Dr. Charlton Bastian, and other observers have noted, that a certain amount of vasomotor paralysis is produced by the operation, resulting in vascularity and increased temperature of the part, and that the improvement in both sensibility and motor power which have in some cases been observed, may be, and probably are, connected with these phenomena.